Meetings

History of Biochemistry and Molecular Biology

Recognizing that the development of modern biochemistry and molecular biology represents one of the supreme intellectual achievements of our time, with far-reaching implications for other sciences and for human affairs, an increasing number of scientists, historians and others have begun to trace the origins and the course of these developments. To consider and promote such endeavors, the Committee on the History of Biochemistry and Molecular Biology of the American Academy of Arts and Sciences brought together about 30 participants in a small conference at the House of the Academy in Brookline, Massachusetts, from 21 to 23 May 1970. Among those taking part were scientists who had been active contributors in these areas of investigation, historians and sociologists of science, and librarians concerned with the gathering and preservation of source material for the future historian.

The participants recognized the important opportunities for the historian in this field today. Many of the leading workers who are responsible for the great advances of the last half-century are still with us and can provide much insight into the nature of the events as they were actually seen by the investigators themselves. The opportunity, however, may be lost if we do not take steps to preserve the unpublished papers and correspondence of the major and some of the minor actors in the drama, and obtain their personal recollections of the events in which they were involved. The lively discussion at the conference helped to promote some of the necessary interchange of ideas between scientists, historians, and archivists, and to suggest further steps that could be taken.

In the opening session Carl F. Cori outlined the history of lactic acid in biology since its discovery by Scheele in the 18th century, including the long and tangled history of lactic acid in relation to muscular activity, and the controversy of a century ago over the relative roles of carbohydrate, fat, and protein in muscular work. S. E. Luria described the early history of the bacteriophage work which Max Delbruck and he initiated; the role of the Cold Spring Harbor Laboratory of a generation ago, with the enthusiastic encouragement of Demerec as director of the laboratory; and the close interplay of thinking and experiment among a small group including Delbruck, Luria, A. D. Hershey, and T. F. Anderson with his electron microscope. When they were not together in person, they were constantly in touch by letter and telephone. Who did the experiment to settle some new idea was unimportant; it was only important that the thing should be done. Among many other points, Luria discussed Avery's work demonstrating that DNA was the transforming factor in pneumococcus—work of which he was well aware even before its publication—and considered why its revolutionary implications did not have a more immediate impact on the phage geneticists.

Taking up at this point, Gunther Stent considered the idea that some discoveries are premature and are, therefore, not appreciated in their time. The work of Mendel is the prime example in biology, but to some extent the concept may apply to Avery's work also. Some have dismissed the whole idea of "prematurity" as essentially a tautology; but Stent, although holding that tautologies are often in fact highly significant, maintained that a discovery is actually premature when it cannot be connected by a series of simple logical steps to contemporary canonical ideas. It cannot be appreciated until a series of other advances has provided a new framework into which the discovery can fit. He also discussed, and rejected, the general view that scientific and artistic creation are fundamentally different—the view that a creation in art or literature is unique and irreplaceable, whereas a scientific discovery, if not made by one man, will surely be made by another within a very short time. Stent believes that there is considerably less of this kind of uniqueness in art, and a good deal more in science, than the common view maintains. These views led to a long and very lively discussion. Robert K. Merton remarked that Stent's views on uniqueness represented the first really novel contribution to the subject that he had heard in 35 years and discussed them in relation to his own studies of repeated duplication of scientific discoveries by different people.

J. T. Edsall, F. J. W. Roughton, A. B. Hastings, and W. H. Forbes considered the growth of knowledge of the role of hemoglobin in the transport of oxygen and carbon dioxide in the blood, beginning with the observations of Christian Bohr and his collaborators in 1904, which for the first time demonstrated cooperative interactions in the binding of oxygen and what would now be called heterotropic interactions of carbon dioxide on oxygen binding. The later work of J. S. Haldane, J. Barcroft, L. J. Henderson, D. D. Van Slyke, and others led to the detailed characterization of blood as an integrated system, highly adapted to its function, primarily because of the remarkable properties of hemoglobin.

R. C. Olby examined the growth of molecular biology at the University of Cambridge and at Caltech, considering the factors and events in the earlier history of these institutions which served to foster the brilliant later developments. Saul Benison and Peter D. Olch discussed the problems of gathering, editing, and preserving oral histories; the value of such histories; and the pitfalls and limtations involved in using them. The historian who sets out to gather the personal recollections of those involved in significant scientific developments must immerse himself beforehand in the work of his subject—not only the published work, but as much of the unpublished background material as possible. The series of interviews that follow may run to 40 or 50 hours, though in many instances a much smaller period suffices. With preliminary preparation, and subsequent editing of the material in the interviews, Benison estimated that the interviewing of three or four people a year is as much as the historian can wisely undertake. He must be able to ask the right questions, be a good listener, and keep his temper.

Several participants reported on their current studies in the history of biochemistry. F. L. Holmes discussed the

controversies on nutrition and metabolism that involved Magendie, Liebig, Dumas, Claude Bernard, and others in the second quarter of the 19th century. A. J. Ihde outlined his extensive studies on the history of nutrition. Stanley Becker presented the early history of vitamin A, including the intense rivalry between E. V. McCollum and the team of T. B. Osborne and L. B. Mendel. R. E. Kohler considered the series of crucial biochemical discoveries which occurred in the last decade of the 19th century and led to the rapid development of biochemistry in our own time: The separation of the zymase system from the yeast cell, demonstrating that fermentation could proceed outside of the living cell, was probably the most crucial event of a whole network of discoveries. John Parascandola reported on his studies on the work of Lawrence J. Henderson, an influential biochemist, physiologist, sociologist, and philosopher of science.

There was much discussion of the opportunities and needs for gathering and preserving the documents that will be essential to future historians of science. Charles Weiner described the research in the history of modern physics being conducted by the American Institute of Physics. The extensive collections of the Niels Bohr Library include, in addition to published material, the correspondence and other unpublished documents deposited by physicists, autobiographies from a considerable number of physicists, a film library, and about 10,000 photographs. Weiner emphasized the importance of coupling research efforts with the collection of documents and the value of holding small, carefully prepared conferences which bring together scientists, historians, and others. Whitfield Bell of the American Philosophical Society agreed that it is generally best for individuals, especially in universities, to deposit their unpublished papers in the archives of their own institutions, provided that these materials are given proper care. When such arrangements are not possible, he indicated that the American Philosophical Society is prepared to receive material from biochemists and molecular biologists for deposit and care in its archives. If these documents are to prove genuinely useful, archives must be well organized and the material must be properly filed and cataloged. Controls on the use of papers must be carefully considered and specified; the library may see fit to impose its own restrictions, even if the

donor of the material does not specify them himself.

Those present at the conference agreed on the importance of urging leaders in biochemistry and molecular biology to preserve such material from their own files. It was recommended that a statement emphasizing the importance of preservation be sent out in the near future to a selected list of leading scientists in the field. Also, it would be highly desirable to issue a newsletter, perhaps once or twice a year, reporting on the location of such material and on other information useful for scholars. Both the American Institute of Physics and the American Philosophical Society publish such a newsletter. Saul Benison urged the compilation of lists of biographical and autobiographical articles that have already appeared. Everett Mendelsohn pointed out that the *Journal of the History of Biology* could publish at least some of the information for a newsletter among its "Notes." J. S. Fruton spoke of the possible role of the American Society of Biological Chemists in promoting these developments. It was agreed that action should be taken on several of these proposals in the near future and that the American Academy and its Committee should endeavor to implement them.

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